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is that it offers a criterion for experimentation in all the fields of the natural sciences in which it is used, and rests on a characteristic of method which is significant and is not brought out by any other term we have. Further, the definition is in general agreement with most of the cases in which the word is commonly applied, although some investigations that have been called experiments would fall under one of my two classes of methods of observation.

It may be added that if such a definition of experimentation in the physical sciences is justified, the use of the term in psychology consistent with its use in these other sciences would not be limited to the field of accurate measurements. Those who insist that the psychological laboratory should be the place for such investigations only, might insist that all other investigations were necessarily imperfect and unfruitful, but they could not refuse to call them experiments.

Frances Hall Rousmaniere.

Mount Holyoke College.

LINGUISTIC ABILITY AND INTELLECTUAL EFFICIENCY[1]

THE factors that go to make up an individual's general intellectual efficiency are perhaps too numerous and too complex for separate treatment by the experimental psychologist, if they do not even defy analysis by him. We generally assume that we can gauge with greater or less accuracy the mental qualities of an individual by a certain amount of purely empirical observation, but the exact sources of our judgment we often find difficulty in stating in a logical manner. An elementary distinction may be made as between intelligence and intellectuality; the former as an inherent quality and the latter rather as a matter of temporary information, for we often conceive of intelligence as the native aptitude for acquisition, and of intellectuality as representative of the amount acquired. Of course, the two are far from necessarily correlated. Whether or not the factor of intelligence, under this definition, be measurable, the writer hopes to discuss at some future time; the present experiments deal purely with the second quality, and then only with reference to actual and not at all to potential conditions.

Now if it be indeed impossible to subject to analytical experimentation the various factors that contribute to this efficiency, yet there is one single mental attribute, itself capable of many modes of experimental approach, that should show, if the proper method of approach be found, a quite close correspondence with general intellectual efficiency. This is the command over language, practically our sole experimental gateway to all the higher mental processes except attention. Nearly all the factors that constitute our intellectuality—as apart from our intelligence—come to us and go out from us through linguistic media. There seems to be no necessity for any detailed defense of this postulate; linguistic ability, if only the proper test of it can be found, should serve as a sound criterion of intellectuality. Such a measure, were it only precise enough, would be, needless to say, of considerable practical interest.

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[1] A report of an investigation begun in the spring of 1906. As it is not probable that the writer will have an opportunity to recur to this problem in the near future it has seemed best to publish the obtained results as they stand.

The memory tests are open to objection for the reason that they can be said to serve as a measure only of what per cent. of the individual experience has been probably retained. The fullness of this experience will vary, as well as its retention will vary, and this factor it is impossible to take into account by means of memory tests. They are dynamic rather than static measurements. In practise, however, it seems as though they failed even here, for the ordinary tests often fail to disclose in men of the highest intellectual ability any special logical or physiological retentiveness. Here also the varying factors of interest and attention are of prime importance, and equally impossible to control.

The association tests are also replete with technical defects. The difficulties in scoring would seem to make the ordinary test of uncontrolled association quite valueless, save for purely qualitative purposes. The special form of this experiment devised by Jastrow, in which the subject writes a fixed number of independent words as rapidly as possible, or preferably, writes as many independent words as possible in a given time, is much more objective in character, and probably altogether the best of the uncontrolled association tests. With proper technical precautions, this test ought, indeed, to afford a measure of the intellect, and be quite valuable where availability as a group test is a factor. As a test of intellectuality on a fine scale, however, the difficulties in scoring would seem almost insuperable.

The tests of partially controlled association, especially those of the synonyms and opposites type, are peculiarly open to this objection when the subject knows that he is being timed. For example, as an opposite of *simple* we obtain from one subject *difficult* and from another *complex.* The second subject takes half as long for the series as the first. We do not know what the first would have done had he taken as long as the second. It is the same difficulty as with many of the motor accuracy tests; two unknown quantities and one equation. The test is valid only when every one has done his best regardless of time. As the education of the subject increases, so do the difficulties in scoring. We should probably disagree violently as to the proper manner of scoring individuals who respectively returned *perpetual, immutable, enduring, fixed, constant,* as synonyms of *permanent.*

Only where the association is so closely controlled as to render but one association possible can the time factor be justly taken into account. The chronoscope and voice-key lend themselves readily to the measurement of single associations. The most important work from this point of view is probably that of Cattell in the *Psychometrische Untersuchungen.* It is the most precise of all the methods, but it has certain technical limitations, and is rather cumbersome for practical purposes, to which the chain reaction is perhaps better adapted.

All valid tests of the higher mental processes should conform to one fundamental requirement; they should give the subject a certain definite thing to do, and admit of objective determinations of done, not done, or how well done. So far as association is concerned, only the time measurements of absolutely controlled associations really conform to this requirement. Otherwise arise the scoring difficulties that are fatal to validity. We can not well apply such an analogue of the method of average error to the higher mental processes because the scale is too ill defined. The association being under complete control, the results should contain only unquestionable rights or wrongs.

Several tests of this sort were devised and employed, among others, in Mr. F. G. Bonser's as yet unpublished investigation into the reasoning powers of children. Blanks were left in simple sentences for which there was only one appropriate word, the subject being required to supply it as quickly as possible. Two opposite words were printed, one above the other, in simple sentences, the subject crossing out that one which would make the statement an untruth. Logical and fallacious reasons were given for certain facts, the subject checking the good reasons. This last may be compared with the absurd sentence test, in which the subject checks those sentences which state as a fact a manifest impossibility; as, "The count paced up and down the garden reading the newspaper with his hands behind his back."

A simple test of the extent of vocabulary has been employed with good results by Kirkpatrick. The blank in the writer's possession contains the opposites test and a request for the definition of words of varying educational implication, but the most significant portion of the test is a selected list of words most of which are considerably above the average difficulty, the subjects being asked merely to indicate the words that they know, do not know, or regarding which they are in doubt. It is unquestionably subject to certain errors in the individual, but for a large number of subjects these tend to compensate, and to remain constant as between groups. As between individuals, where the test of intellectuality has its greatest practical application, a strictly empirical demonstration of its validity will be desirable.

All of these tests, however, necessarily lack the objective character of a strictly motor test of linguistic ability, provided a satisfactory test of this nature can be devised. This it has been the writer's attempt to do. That only a very modest degree of success can yet be claimed the figures given will show, but the attenuated correlations which do appear indicate some possibilities in this direction.

Experimental psychologists have two ways of approaching problems of this nature. We can say, for example, ' let the *a-t* test measure perception of weight,' and then assume individual differences therein as indicated in the test. This procedure may be sometimes necessary, but it has been too frequently followed; certainly many of the conventional laboratory tests do not measure what they are given to measure, or else they measure a great deal besides. The other method is to empirically select individuals with known differences in the trait we wish to measure, and to seek a test which shows correlation with these differences. Before applying a theoretically devised measure to wholly unknown individuals, we should first test our tests.

The writer selected six individuals according to their degree of objective intellectual performance. These are denoted according to this degree, *A, B, C, D, E, F*. The differences between *A* and *B, B* and *C,* are clearly marked; that between *C* and *D* is largely subjective. *E,* however, is distinctly below *D,* and *F* below *E*; in fact, the difference between *E* and *F* may well be greater than that between *E* and *A*. The individuals are all adults, and the writer would estimate that they occupied, intellectually, a range of from the upper tenth of the distribution to about the middle.

As the problem was preeminently a search for a test many conditions are imposed that turned out valueless, and in some cases even impaired the significance of the figures that gave results. The writer does not wish therefore to be held accountable for resultless peculiarities in the presentation of the experiments, whose significance, or lack of it, could not have been determined in any other way.

The tests were concerned mainly with the speed of reading aloud. In the first experiment, passages I., II., III., IV., VIII., IX., X. are from scientific works, but only passage III. is at all complex in

TABLES

A, *B*, *C*, etc., *1*, *2*, *3*, etc. = subjects. I., II., III., etc. = passages.

TABLE I.

	A	*B*	*C*	*D*	*E*	*F*
I.	31.4	32.8	34.4		28.2	42.4
II.		36.2	35.4	36.8	29.4	44.8
III.	32.2		36.2	38.8	30.6	47.2
IV.	32.2	34.6		42.2		
V.	34	32.4	39	35.8	29	46
VI.	34.4	35.6	36.6	41.4	34.6	44
VII.	33.4	35.2	36	42	36.2	54.4
Av.	33.1	34.5	36.4	39.5	31.3	46.5
M.V.	.7	1.2	1.2	2.3	3.2	2.8

TABLE II.

I.	27.6	28.8	35.4	33.8	24.4	29.8
II.	27.8	29.4	37	33.6	24.6	31.2
III.	29.4	31.2	39	36.6	26.4	31.2
IV.	26.4	29.8	36.4	35.6	25.6	29.4
V.	25.4	28	35	33.6	25	41.8
VI.	29.6	21.6	36.6	35.4	28.4	38.4
VII.	27.8	30.8	37.6	37.6	31.2	45
VIII.	31.2	32.2	42.2	41	32.4	45
IX.	31.2	24.6	29.6	29.2	24.6	36.4
X.	29.4	28	33.8	34.6	24.4	48.8
Av.	27.3	29.4	36.3	35.1	27.1	37.7
M.V.	1.8	1.7	2.4	2.1	2.1	5.9

TABLE III.

	Av_1	Av_2	M. $V._1$	M. $V._2$
A	2	2	1	2
B	3	3	2.5	1
C	4	5	2.5	5
D	5	4	4	3.5
E	1	1	6	3.5
F	6	6	5	6

TABLE IV.

	A	*B*	*C*	*D*	*E*	*F*
I.	2	3	4	5	1	6
II.	2	4	3	5	1	6
III.	2	3	4	5	1	6
IV.	2	3	4	5	1	6
V.	3	2	4	5	1	6
VI.	2	3	4	5	1	6
VII.	1	2	4	5	3	6
VIII.	1	2	5	4	3	6
IX.	5	1.5	4	3	1.5	6
X.	3	2	4	5	1	6

TABLE V.

	Av.	M. V.		
1	19.6	.85	1.5	3
2	27	.3	3	2
3	22.2	.65	7	1
4	27.8	2.45	9	10
5	27.6	1.35	8	6
6	23.1	1.1	4	5
7	19.6	.95	1.5	4
8	26.8	1.85	6	8
9	28.3	1.95	10	9
10	23.5	1.7	5	7

TABLE VI.

	C	*N*	*R*
1	21.8	15	.69
2	19	14.4	.75
3	24	14	.58
4	17.2	16.2	.94
5	23.4	16.4	.71
6	27.4	15.6	.57
7	25	21.4	.86
8	24	17.2	.72
9	26.2	17.4	.66
10	24.2	13.4	.54
11	18.8	16.4	.87
12	23.6	18.4	.78
13	28	19	.67

thought; the remainder are from works of fiction. The passages are all one hundred words in length. Each subject first read aloud V., VI., VII. at subjectively normal rate, also three of I., II., III., IV. as indicated in the table. The fourth was read as rapidly as possible. Upon this occasion these passages were seen for the first time. On a subsequent date the subject read the passage previously read at rapid rate thirty-seven times in succession as rapidly as possible, and on subsequent dates each of the other first four passages was so read forty times, giving in all a practise curve of forty most rapid readings of each of passages I., II., III., IV. Later the subject again read at subjectively normal rate the whole ten passages, VIII., IX. and X. being now seen for the first time.

Table I. gives in seconds the time, average and M. V. of the six passages read at the first sitting, and Table II. gives the time, average and M. V. for the ten passages read at the second sitting, all at subjectively normal rate.

In both series there will be observed a general tendency for both average and M. V. to increase as we go down the scale. In Table I. the averages increase regularly save for the important exception of subject *E*. With this same exception, now in the other direction, the M. V.'s also increase regularly. The situation is reversed for *C* and *D* in Table II., but it has been noted that their positions are subject to a large probable error. However, if we compare *C*'s record in the two tables, the influence of diurnal variation would seem to be indicated. Subject *E* again stands higher than he should. His subjectively normal and most rapid rates of reading approximate one another more closely than is the case with the other subjects. The considerable practise with series I., II., III., IV. shortens all the repeated readings somewhat, whether among those practised or not. It will be noted that this practise has in subject *F* formed two species in the second readings, leading to an extra large M. V. The M. V. of I.–VI. is very small, but that of V.–X. is very large, larger than that of any of the other subjects, as is also the average of the two M. V.'s. Other results of the experiment, as the ratio of the normal and rapid reading, etc., do not appear to correspond so closely with the empirical series. Table III. gives the results in synoptic form by stating relative positions instead of the figures themselves. Thus *E*'s average was quickest for both series of readings, while his M. V. was greatest in the first and shared third and fourth places with *D* in the second. On the whole, the first readings are better than the second, and except for *E* the average is superior to the M. V., though both should be known. The character of the practise curves seemed to be related to other mental traits in the individuals, but had no traceable connection with the intellectual. Table IV.

gives the relative position of each subject in the rate of reading each passage. Thus I. was read by subject *E* fastest, *A* next and *F* slowest. Considering the small absolute differences, it seems worthy of note that the rates are so constant.

In reading aloud a wholly unfamiliar language, it would make no difference to our speed whether the words were in logical order or not. In so far as we did derive their sense should our speed with the logical order be increased over the illogical. Upon this theory a test was devised in which the subject read as rapidly as possible series I.–IV., and then read as rapidly as possible the words composing them in nonsense order. Nonsense syllables would have been preferable, as the writer now realizes. The subjects differed from those in the first test, only positions 1 and 2–3 being fairly certain. The differences are also very much smaller, and for the most part wholly subjective in estimate, as it did not appear worth while to obtain accurate judgments of relative position from a number of graders. The ratios of the two times, while showing much individual difference, appear so entirely unrelated to intellectual performance that it has been deemed hardly worth while to print them. They range from .47 to .97. Table V. shows in the first and second columns the results of this experiment for average and M. V., in the third and fourth the relative positions for average and M. V. The differences are not nearly so well marked as in the previous test, but the method should hardly be discarded on this account, for the number of observations and the differences in the subjects are very small, while correlation is by no means altogether absent. Striking exceptions to the rule, as *E* in Tables I. and II., are subjects *4* and *7*.

During the course of the *Psychometrische Untersuchungen* the observation was made by Cattell that a comparatively illiterate porter used as a subject required a longer time to read the name of a color than to name it, while the cultured subjects could read the name more quickly than they could name the color. The writer arranged a chain-reaction experiment with the four colors, red, green, blue, yellow, fixing twelve red and twelve green, thirteen blue and thirteen yellow slips upon a white background in a frame, while two similar frames contained the names of the colors in opposite orders. The color slips were so cut and arranged as to occupy the same average space as the names. The time was taken which the subject required to read the colors, and then their names in reverse order, and the ratio calculated. This experiment was performed upon thirteen subjects partly identical with those who had undergone the previous tests, and covering about the same range of intellectual ability as in the first test described. As shown in Table VI., all subjects name the words faster than the colors. If we disregard the ratio, and take the time in either test singly, the experiment is brought into line with the speed of reading tests. For a series of single observations, the result is rather suggestive. Important isolated exceptions occur for the colors in subjects *6* and *11*, and in *10* for the names. The ratios, however, are simply chaos, just as were those in the previous experiment. The writer has examined the ratios with reference to other traits in certain of the subjects, but no correspondence with any factor of character is observable. The two perhaps most temperamentally similar subjects of the experiment—*3* and *4*—are nearly the most different in their ratios. There seems to be good theoretical ground to discredit the ratio as a desirable form in which to state results of this nature, for besides being highly subject to attenuation, it puts a premium on the subject's doing badly in one of the tests.

Most of the results are therefore negative. Two factors appear related to intellectuality: the speed of absolutely controlled association and its constancy relative to that of other individuals over a similar series. High speed and low variability go with intellectuality, low speed and high variability with the reverse; both high or both low are doubtful, but seem to be associated rather with the latter. To all these generalizations, however, one must be prepared for striking individual exceptions.

FREDERIC LYMAN WELLS.

COLUMBIA UNIVERSITY.

REVIEWS AND ABSTRACTS OF LITERATURE

Die Erkenntnistheorie der Naturforschung der Gegenwart: H. KLEINPETER, Ph.D. Leipzig: Johann Ambrosius Barth. 1905. Pp. xii + 156.

"The work before us agrees in general with the views as to the nature of knowledge expressed by Mach, Kirchhoff, Hertz, Stallo, Clifford, Pearson, Ostwald and Cornelius" (p. xi). It is dedicated to Ernst Mach. A theory of knowledge has developed in the second half of the last century on the basis of the exact sciences, and the author proposes to give it a much-needed systematic presentation. His method is neither historical nor critical, although he assumes a very critical attitude toward the science of metaphysics and the traditions of philosophy. Our only means of testing the correctness of views is through further experience, and this is a merely negative test, telling us only what is not true, but never what is. "Where this test does not apply, as in judging philosophical opinions, all genuine progress has been wanting for more than